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also find that in their reproductive organs they form a more or less natural transition from the cryptogams to the phænogams, between which they place them. This result is most gratifying to the palæo-botanist, for nearly all works on fossil plants give the gymnosperms this position at the base of the phænogamic series, so sagaciously assigned to them by Brongniart. They have been compelled to do this in the face of the prevailing botanical systems, because this is the position which they are found to occupy in the ascending strata of the earth's crust. It is astonishing that botanists could have remained so indifferent to such a weighty fact, and it is certainly most instructive to find the geological record, so long unheeded, confirmed at last by the facts revealed in living plants. There is no evidence that those who have thus confirmed it were in the least influenced by it, since Sachs and Caruel are as silent respecting palæontology as De Candolle or Bentham.

The founders and perfecters of the prevailing system of botanical classification have not been influenced to any marked degree by the idea of development in vegetable life. Few of the earlier ones had ever heard of development, except at least as a visionary theory. This system had become established long before the doctrine of the fixity of species had received a shock, for although Lamarck, himself a botanist, had sown the seed of its ultimate overthrow, still it required half a century for this seed to germinate, and it was during this half century that the Jussiæan system was supplanting the Linnæan and gaining a firm foothold.

Shaking off, for the time being, all fixed allegiance to any system, let us glance for a moment at the lesson which vegetable palæontology now teaches upon the subject of development.

(To be continued.)

——:o:——

ON THE VERTICAL RANGE OF CERTAIN FOSSIL SPECIES IN PENNSYLVANIA AND NEW YORK.

BY PROFESSOR E. W. CLAYPOLE.

THE Second Geological Survey of Pennsylvania has recently published a report on Montour, Columbia and several other counties, written by Professor I. C. White, of the University of West Virginia. While engaged in the work Professor White requested the writer to determine for him the fossils which he collected. This was done, and the results were embodied in the volume in question, G_7 of the Pennsylvania reports.

In the preface to the volume Professor James Hall, of Albany, has, through the State geologist, honored the palæontological portion of the work with certain criticisms which call for a few remarks. The science of palæontology advances so rapidly that statements which were perfectly correct at one time often cease to be so, and from this ground, if from no other, the criticisms contained in the preface of Professor White's report require some notice.

I.

It will not be necessary at any length to discuss Professor Hall's first remark concerning the dividing plane between the Chemung and the Catskill. In the present state of our knowledge this must be largely a matter of opinion. One observation will suffice. Whatever may be the fact in Montour and Columbia counties, there is no doubt that in Perry county, with which the writer is better acquainted, spirifers, unbroken and with both valves in contact, are found about 1000 feet above red sandstone beds holding the scales of Holoptychius or Bothriolepis or both.

II.

In regard to the spirifers of the Chemung, certain statements are made which are not in harmony with facts which the writer has observed in Middle Pennsylvania. For instance, on page xx we read:

> " How is it possible to credit such a topsy-turvy appearance of the three species of Spirifera which, outside of Pennsylvania, have been found (1) never in any but Chemung rocks; (2) confined each to its own horizon; and (3) always in a fixed order from above downward, thus :
>
> *Spirifera disjuncta* horizon (*S. dj.*).
> *Spirifera mesocostalis* horizon (*S. mc.*).
> *Spirifera mesostrialis* horizon (*S. ms.*)."

On page xxi we read further :

> " *Spirifera disjuncta* and *Spirifera mesostrialis* form an impossible ? conjunction."

This assertion must be, to say the least, somewhat hazardous even with the interrogative point.

Again, on the same page :

" Professor Hall has never seen any two of the three species coëxisting in the same stratum or at the same horizon or outside the limits of the typical Chemung. He would not be surprised if *S. mesocostalis* were found to ascend high enough above its proper horizon to mingle with *S. disjuncta.* But he cannot comprehend how *S. disjuncta* and *S. mesostrialis* should be found together."

In regard to this point the experience of the writer in Pennsylvania has led him to very different conclusions. So far from being strictly limited to single horizons, these three species appear to range with great latitude over each other's territory. This is especially true of the two lower fossils, *S. mesocostalis* and *S. mesostrialis.* It is no doubt true that each species is specially abundant in certain beds or zones, but while in a measure characterizing these, it spreads both above and below them, and mixes with one or both of the others.

These statements are made concerning Middle Pennsylvania and have no reference to any other district. The writer's experience among the rocks and fossils of New York is not sufficient to enable him to speak with any authority concerning them, and to these it may be supposed Professor Hall is chiefly referring in the passage quoted above.

Since the publication, however, of the report of Professor White, and called forth by its preface, a letter has appeared from the pen of Professor H. S. Williams, of Cornell University, in which, referring to these remarks of Professor Hall, he says:

> " While the statement cited may express the general rule as to the occurrence of the species in New York State, there are specimens in Cornell University museum which do not bear out the statement.
>
> " In the first place the two species, *S. mesostrialis* and *S. mesocostalis*, are found associated in the same stratum at Ithaca, N. Y., both in the *mesostrialis* zone and in the *mesocostalis* zone. Several instances can be shown where they occur on the same slab.
>
> " From a higher horizon in New York State, and from several localities, either of these species may be found associated with *S. disjuncta*, and I have obtained each of the three species from the original Chemung locality at Chemung narrows.
>
> " In the museum collection is a small slab containing beautiful representatives of *S. disjuncta* and *S. mesostrialis ;* the latter preserving ' the fine radiate striæ with delicate concentric crosslines' all over the surface of the shell, and with ' the broad median fold without a depression ' which are described as distinctive characters of the species (Pal. N. Y., Vol. IV, p. 243).

"The other specimen, only a couple of inches distant, has the characteristic plications on the medial fold, and with the surface equally well preserved, shows not the least trace of radiate or concentric striæ, unmistakably indicating *S. disjuncta.*

"From the same locality, though not on this individual slab, are specimens of both varieties of the so-called *S. mesocostalis*—the large coarse form with angular plications and reduplicated fold, and the more finely plicated form with prolonged hinge line which is more characteristic of a lower horizon."

From these statements of Professor Williams we find that the three following inferences are now known to be true for *New York State :*

1. That *S. mesocostalis* occurs associated with *S. mesostrialis.*
2. That *S. mesocostalis* occurs associated with *S. disjuncta.*
3. That *S. mesostrialis* occurs associated with *S. disjuncta.*

These are all the possible combinations of three things taken two together. Consequently these three species are mingled in New York State in all possible ways in which two of them can be combined.

In order to strengthen, if possible, by cumulative evidence the argument founded on these clear and definite statements, the writer applied to Professor R. P. Whitfield, of New York, asking if the collection of the New York Museum of Science, of which he is curator, could supply any facts that would support the views on the range of these fossils which are here maintained. The greater part of that collection was made and the specimens named by Professor Hall, so that no doubt regarding the identification of the species can be entertained. Professor Whitfield has stated in reply:

1. "On a small block of sandstone containing the specimen of *S. mesocostalis*, figured in the Palæontology of New York (Vol. IV, Pl. XL, Fig. 6), there is an imprint of a part of a very characteristic specimen of *S. disjuncta.*
2. "Another specimen measuring four inches by three, from Schoharie county, N. Y., and bearing Professor Hall's ticket (No. 302), contains one of the specimens of *S. mesostrialis*, figured in the same plate (Pl. XL), and also casts or imprints of three individuals of *S. mesocostalis.*
3. "Another small slab marked, in Professor Hall's writing, 'Tioga county, N. Y.,' contains two dorsal valves of *S. disjuncta* and two ventrals of *S. mesocostalis.*
4. "Another small block, bearing Professor Hall's ticket, shows dorsal valves of *S. mesocostalis* with specimens of the long mucronate hinged form of *S. disjuncta.*

5. "One from Vandermark's creek with large examples of *S. disjuncta* contains a ventral valve of *S. mesocostalis.*

6. "Another bearing Professor Hall's ticket (279), from Cayuta creek, N. Y., has *S. disjuncta* with *S. mesocostalis.*"

Professor Whitfield also adds:

"On one small specimen weighing ten or twelve ounces and bearing the label 'Cayuta creek,' with characteristic specimens of *S. mesocostalis* and *S. disjuncta,* there is an imperfect imprint of the medio-dorsal part of a ventral valve presenting the features of *S. mesostrialis* as it occurs at some western localities [of N. Y.], but its outline is too imperfect to admit of positive identification."

The testimony obtained from the collection in New York, therefore, agrees in every point with that given in Professor William's letter, and suggests in addition the possible association of all the three species at Cayuta creek.

Yet one fact more may be given. Professor H. W. Geiger, of the U. S. Geological Survey, has informed the writer that he possesses a slab from the Chemung of Virginia on which *S. mesostrialis* and *S. disjuncta* are lying side by side.

In the face of these facts it is quite impossible any longer to maintain the sharp delimitation of the horizon of these three species above and below. It is evident that though characterizing, probably in some places by their abundance, certain zones, they are not by any means limited to these zones, but invade each other's territory to an undefined extent.

This result is more in harmony than its opposite with our present views of the progress of life on the globe. We have no reason to believe that species came suddenly into ascendancy and then as suddenly went out, especially at times unmarked by any catastrophe, as was the case in the Chemung era in Middle Pennsylvania and New York. On the contrary, the general belief in evolution involves the special belief that every species has, in ordinary circumstances and barring accident, had its time of rising, culmination and decline, during which its life has overlapped the life of other kindred and perhaps derived species. Nothing is less likely, *a priori,* than that three species of spirifer should lie like three drawers in a geological cabinet one above another. Nothing is more likely than that each should occur in gradually increasing numbers until it reached its maximum, and then in gradually diminishing numbers until it died away. Each may then distinguish certain horizons, as Professor Hall describes them, but without sharp and decisive limitation to these horizons.

It is farther possible, and not improbable, that these zones of maximum abundance hold the same relative positions to one another in New York and in Pennsylvania. This, however, must be decided by closer study. Nothing that the writer has seen or published is antagonistic to such belief. But even if the zones of maximum abundance should not hold the same relative positions to one another in different parts of the country, we are not driven to the alternative of asserting a " subversion of specific types in vertical range " (p. xxvi). Given the three species, or indeed any two of them, living side by side through the greater part of the Chemung era, and we need only admit that local conditions favored here the one and there the other, and the whole difficulty disappears. It is purely imaginary. It is scarcely probable that identical conditions of life existed contemporaneously over so great an area, it need not consequently shake our belief in palæontology if the result should show that in Pennsylvania and Virginia, or in States still more distant, the zones of maximum abundance hold an order different from that which Professor Hall has laid down for New York.

Another fact may be mentioned in this connection which, though not directly connected with the argument, yet serves to show that the lines of delimitation bounding the range of fossil species cannot be laid down as definitely as has been done by some palæontologists. *S. lævis* is one of the characteristic fossils of the Portage group in New York State occurring neither above nor below, so far as the writer is aware. Yet in Middle Pennsylvania it has not been found in the strata occupying the position of the Portage of New York, and holding other Portage fossils. But it does occur higher up, in the Chemung proper, and in company with *S. mesocostalis.*[1] *S. lævis*, it may be added, is a well characterized species in New York, and is therefore readily recognized.

III.

Passing on to another topic, we find on page xxii:

" *Orthis tulliensis* has certainly never been seen before in the Chemung two hundred feet above the Genessee (*i. e.*, three hundred feet above the Tully limestone), nor in the company of *S. mesocostalis.*"

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[1] This is possibly *S. mesostrialis*. The specimen is only a fragment.

This statement may be strictly true, but it may be urged, on the other side, that there is no distinct character by which *O. tulliensis* can be separated from *O. impressa.* No doubt two forms can be picked out as types of the so-called species, but their variation is considerable and they run into one another. It would be exceedingly difficult, perhaps quite impossible, to separate accurately a mixture of the two species if the horizons of the specimens were unknown. We seem here to have an example of a vicious reasoning of which palæontology affords not a few similar instances. The Orthis from the Tully limestone is called *O. tulliensis;* that from the overlying Chemung is called *O. impressa.* But barring this difference it would be hard to draw a line between them. The practice of giving different names to fossils simply because they come from different horizons has been carried too far. It has created artificial barriers in the way of tracing the evolution of species and the connection of strata. Neither geological nor geographical separation is sufficient reason for distinguishing by name fossils between which no clear structural difference can be pointed out. The cases are few, especially among the Testacea, that justify resort to the argument used by the late Professor Meek, "that if the whole structure of the animal had been preserved doubtless some distinctions would be found which do not exist in the fossil."

Referring to the extract given above from the preface of G_7, Professor Williams says in the letter already quoted:

"In regard to *Orthis tulliensis* it may be said that the common Orthis occurring at the base of the Ithaca fauna within a few hundred feet above the Genessee shale (less than 500) resembles at its first appearance *Orthis tulliensis* in form and general character, though for distinctness it may be appropriate to call it a variety of *Orthis impressa,* since a little higher and in the same fauna the typical *Orthis impressa* appears in abundance.

"Still there are specimens in the collection, from the lowest zone, which it would be difficult for any one to distinguish by microscopic or macroscopic characters from *O. tulliensis.*"

There is therefore *in New York* an Orthis which cannot be distinguished from *O. tulliensis,* occurring not at 200 feet only, but at a yet greater height (less than 500 feet) above the Genessee shale.

In regard to the association of this fossil with *S. mesocostalis,* Professor Williams adds:

"I have no single slab containing this form with *S. mesocostalis,* but the latter is found both above and below the stratum containing the Orthis."

IV.

Reverting to the preface of G_7, we read on page xxii this remark, apparently by the State geologist:

"*Halysites catenulatus* [the common chain-coral] is found quite out of its natural place; at one locality only it is true, but in such abundance as to make up a large proportion of the twenty feet of beds through which it is found."

"'Sir W. E. Logan recognizes it as low as the *Trenton limestone*, and I have seen a form or variety of the same in the *Hudson River group* of Green bay,[1] but no one has ever before found it above the Niagara.'"[2]

Professor Lesley adds very justly:

"Whatever may be the difficulty of distinguishing *S. disjuncta* from *S. mesostrialis*, it is quite impossible for any one to mistake *Halysites catenulatus*, the characteristic form of the *Niagara limestone* all over the United States and Canada, and also of the corresponding *Wenlock* of Europe.

In these passages there are obvious traces of the vicious reasoning already condemned. The "natural" place of a fossil is surely that in which it is found. Its "canonical" place may be very different. Now of the occurrence of *Halysites catenulatus* in the Lower Helderberg limestone at the place mentioned by Professor White, there cannot be the slightest doubt. The obvious character of the fossil, such that every tyro in palæontology is acquainted with it, precludes all chance of mistake in its identification. The position of the limestome bed in which it was found is equally decisive against stratigraphical error. It overlies several hundred feet of red shale and sandstone, which are the equivalent of the Onondaga shales of New York—the Salina shale or Salt group, as they are often called.

The correlation of these has been satisfactorily determined in Middle Pennsylvania, and the evidence, both stratigraphical and palæontological, may be seen in the writer's report on Perry county (F_2). It is proper to add here that the above statement concerning the place of the bed is made from personal knowledge, the ground having been visited for the purpose in company with Professor White.

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[1] Some inconsistency exists between this statement and that on p. xxv. "Professor Hall has given himself infinite trouble to obtain the data on which rests the proof that *H. catenulatus never* occurs except in *Niagara* and *Clinton* rocks."

[2] Letter from Professor Hall.

Immediately above the limestone bed in question comes the Oriskany sandstone, so that the evidence for position is rendered doubly sure, and it would be idle, in the face of all the facts, to entertain any remaining doubt or suspicion regarding the horizon or the species of the fossil. It is *Halysites catenulatus*, and it is found in the Lower Helderberg limestone.

Halysites catenulatus ranges in England from the Llandeilo up to the Wenlock, and in America from the Trenton to the Niagara, by consent of all geologists. At this horizon its disappearance has been assumed. But the determination here maintained carries it up through about 1200 feet of strata, and extends its specific life through a correlative lapse of time.

In the consideration of these facts it may be remarked that the very ease and certainty with which this fossil can be recognized appear to the writer a possible source of error. Being abundant at numerous places in the Niagara limestone, it forms a convenient fossil reference, and its range has been, by tacit consent and perhaps with the aid of influential names, assumed to be limited upward by the summit of the Niagara. It has thence become the practice to refer all strata containing it to the Niagara group, and the two have become closely associated. Yet the foundation for so strict a delimitation is as unsatisfactory as in the cases previously examined. It is in fact another instance of vicious reasoning. *Halysites catenulatus* occurs very frequently in Niagara rocks, therefore all rocks yielding *Halysites catenulatus* are of Niagara age.

If this fallacy be avoided and the upward range of Halysites be admitted, its occurrence in the place and on the horizon mentioned by Professor White (in G_7) instead of being "a serious matter for Pennsylvanian geology" (p. xxiii), may be a matter of importance to the geology of some States outside of Pennsylvania, and may even cause the removal to a higher level of some strata which have been hitherto placed in the Niagara group by the hasty deduction above condemned.

For example, it is just possible, to say no more, that the occurrence of this fossil in the cement beds, as at Kingston, N. Y., may be hereby explained. If these cement beds are the same as those usually included under that name, they should lie in the Lower Helderberg series and not in the Niagara, as often stated. Moreover a reference to Professor Hall's geological map of New

York State (Geol. N. Y., Vol. IV, 1843), and also to that of the Geological Survey of Canada, 1863, will show that no outcrop of the Niagara is represented within a hundred miles of Kingston, the color representing that group ceasing near Utica, while the Lower Helderberg continues and underlies the town. It is hard to resist the conclusion that we have here an instance of correct stratigraphy overruled by incorrect palæontology, and that so far from its occurring only in the Niagara this is an example in *New York State* itself of the occurrence of Halysites in the Lower Helderberg limestone.[1]

V.

From the facts and arguments here set forth the general conclusion must follow that all attempts to confine the range of species within certain arbitrary lines are attempts that are not likely to succeed. The geological record as written in the rocks is a record of life. Evolution teaches us that life advanced by slow stages from species to species; that as one died out another took its place. Evolution knows nothing of breaks or of hard and fast planes of limitation in the range of species. All such planes are indications of imperfection in the geological record, perhaps evidence of catastrophes on a small scale, but they are no proof of widespread disaster and destruction. And in proportion to the advance of our knowledge we must expect, on the principles of evolution, to see these breaks one by one disappear, and these lines be one after another effaced until the record of the rocks is in harmony with the record of life which it represents. That this harmony will ever be perfect is unlikely, for the rocks will never give up all their dead, but that it will one day be much more nearly complete than now is axiomatic, in the face of the continual discovery of missing chapters in the history supplying missing links in the chain of life.

The artificial systems of palæontology which have been constructed by the faithful, earnest and devoted labors of the students of the science are but temporary. They are invaluable aids to the progress of the work, but they are only the means and not the end. Progress will be hindered if these systems are allowed to cramp and fetter us. If preconceived notions of what *should be* are suffered to blind our eyes to what really *is*, our palæontology would itself become a fossil, as dead as the trilobites of palæozoic days. Our subdivision of the geological column into Hamilton, Chemung, Catskill, &c., or even into Cambrian, Silurian and Devonian, &c., is simply a device to assist memory and classification, not to represent actual and separate creations. And with every new discovery we must expect to see the lines and planes that separate these imaginary groups and systems become less and less clear until they are fused into a whole whose parts shade into one another like the colors of the rainbow. Nature is larger than our systems, and our knowledge of fossil nature must some day outgrow our artificial "canons of palæontology." But in all such cases nature cannot be warped to our "canons," our "canons" on the contrary must bend to the facts of nature.

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[1] On p. xxiii (G_7) an error appears which causes some confusion. "*S. macropleura* is the earliest spirifer which shows ribs in the medial series (sinus ?), and it recurs nearly unchanged in the Subcarboniferous Chester limestone of the West." *S. macropleura*, as may be seen by looking at Professor Hall's figures (Pal. N. Y., Vol. III, Pl. 27), has no ribs in the medial sinus. Nor does it recur nearly unchanged in the Chester limestone of the West.

The writer cannot conclude without, superfluous as it may seem, adding a word to express his sense of the value of the labors of Professor James Hall in American palæontology. He has laid broad and deep foundations for future workers. How numerous or industrious soever they may be, they must always acknowledge that they are building over his beginnings. That some errors should creep into work so great and varied is to be expected. But compared with the grand whole, they are insignificant. In pointing out and correcting a few of these errors in the foregoing pages, nothing is farther from the writer's wish than to seem to depreciate Professor Hall's labors. The facts and misstatements here criticised are mainly details—mere spots on the face of the sun—but for that reason the more worthy of attention and scrutiny.

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ANCIENT ROCK INSCRIPTIONS ON THE LAKE OF THE WOODS.

BY A. C. LAWSON, M.A.

WHILE prosecuting a geological survey of the Lake of the Woods last summer, I observed upon the rocks, at two places not far distant from each other on the shores of the lake, ancient inscriptions which may be of some interest to those who are engaged in gathering up and weaving together the scattered